

Review comments for “New insights into the atmospheric mercury cycling in Central Antarctica and implications at a continental scale” by Angot et al., acp-2016-144

General comments:

This paper describes year-round measurements of Hg(0) in the atmosphere and snow on the Antarctic plateau along with ancillary measurements. These comprise a novel data set that is very valuable for understanding the global atmospheric (and cryospheric) mercury cycle. Given the value of these data and the difficulty in duplicating them, I would encourage the authors to make the complete data set available in some capacity (e.g. as a supplementary file, or a link to a data repository) in order to aid modellers, etc., in using these measurements to advance further research.

The analysis and interpretation is largely sound, with a few gaps as identified in the comments below. I do agree with the first reviewer that the organization of the Results and Discussion could be improved. I recommend the publication of this paper after the minor issues discussed below have been addressed.

Specific comments:

I. 38-39: “according to observations at coastal Antarctic stations” is vague; this is used elsewhere in the paper (l. 488) and is not very enlightening. Can you summarize the evidence you are using to draw this conclusion? Perhaps in Section 3.7? There are references there but the observations are not described.

I. 53: “rapid deposition” is relative. You later describe a reservoir of gaseous Hg(II), which can hardly be expected if the deposition lifetime is very rapid.

Section 2.3: What is the estimated precision of your Hg(0) measurements?

Section 3.1: (a) This is titled “seasonal variation” but mostly summarizes annual values and spatial/vertical differences. (b) Can you identify what the \pm values are? Standard deviation? Confidence limits on the mean? (c) What statistical test was used to determine that your values were lower than the Troll and Neumayer – Mann-Whitney as well?

How did the 25 cm inlet met tower values compare with the 50 cm and 10 cm snow tower inlet values overall? Fig. 6 suggests there was some offset between the met tower and snow tower inlets, at least in winter and possible spring/fall. Is the sampling coverage the same? Could there be an effect of heated/non-heated lines, or the length of the sampling lines?

Fig. 3: Why did you choose the 25 cm inlet to show? Can you add a time series or two (shallow/deep) for the snowpack data?

Fig. 4: I only see error bars on a few points. Are these the only ones with replicates? How many replicates were done in those cases? A line or two in the caption to explain this would be helpful.

Section 3.2: You refer to “continuous” oxidation in the summer. What do you mean by that? It is clear there is net oxidation, but I am not sure you have shown it is continuous.

Fig. 7: This figure is a nice attempt to visualize the data, but it is rather confusing. Does the shading represent some sort of unspecified interpolation? Do the top boxes represent the met tower and the bottom the snow tower? In that case, why do the top boxes extend down below zero and the bottom ones not go up to 50 cm? If not, why don't the top and bottom agree within the overlap region?

Fig. 8: I think this figure is not crucial to the paper, since it is only being used to support a suggested mechanism for a single extreme value. I think you can make that suggestion without an additional figure, though it is up to your discretion.

Section 3.3.2: You mention a shift from oxidation to reduction at the beginning of winter, but it would be very helpful to see the time series of Hg(0) at depth (as mentioned above) – is it a sudden drop to a stable “winter” value, or is there a longer trend over the winter to accompany the atmospheric decline?

Section 3.4.1: Your summary is a bit confusing (ll. 339-343). I think you are saying by “continuous” that (i) has a week diurnal cycle and by “important” that (iii) has a strong diurnal cycle, resulting in the observed concentration pattern. Can you say this more clearly? I'm not sure what “important” means in (ii). Clearly it is important to the surface snow THg, but I'm not sure how this is related to the Hg(0) diurnal cycle.

Section 3.4.2: Where is this Hg(0) building up from? Presumably the snow, but it's not mentioned. Why is the fall concentration higher than spring? The reservoir of Hg in the summer snow?

Fig. 9g: What inlet is the Hg(0) cycle from?

Section 3.5: It's a bit odd to refer to Fig. 10e first. I suggest you rearrange the figure to make this 10a.

Section 3.6: (a) Given you have a single winter of data, and the decline is not seen at the other stations, can you eliminate instrument drift (e.g. trap poisoning)? Were the external calibrations before and after the winter consistent? (b) Why do you not include dry deposition of Hg(0) as a possible mechanism for this decrease? Given the low BL, what deposition flux would be needed to remove the observed amount of Hg(0)? How does this compare to other observations/calculations (Cobbett et al 2007, Zhang et al 2009)? I think it's quite similar. It would also account for the gradient in the decrease (3.6.2). (c) Speaking of which, you don't report in 3.1 if there are any seasonal differences in the three met tower inlets. Your discussion of the winter data suggests there would be. (d) This section is poorly organized. I suggest removing the sub-sections since you basically discount the gas-phase reaction without doing so explicitly. (e) l. 402: Why don't you report your O₃ data instead of (or as well as) referring to another paper? Also, are there O₃ data at Troll or Neumayer that suggest that a winter reaction with O₃ would not also happen there?

Section 3.7: A bit more detail about the observations that are attributed to transport from the plateau (ll. 457-462) would be helpful, as mentioned above.

Section 4: (a) I'd like to see a mention of the intriguing winter subsurface Hg(0) peak in here. (b) Maybe change “heating” to “snowpack ventilation” or “ventilation and heating” in l. 479. (c) In l. 481 I would change “likely” to “possibly”.... And do you really think gas-phase oxidation is even that likely? Your

earlier discussion suggests not. You may need to add dry deposition of Hg(0) as well, depending what you find.

Technical corrections:

l. 32: change “never been observed” to “not been reported”

l. 46: change “contamination” to “contaminant”

l. 246: change “bound” to “bind”

l. 315: change “significant and daily” to “significant daily”

l. 316: change “all along” to “throughout”

l. 358: suggest changing “explosions” to “so-called ‘bromine explosions’” to avoid leaving unfamiliar readers with the impression there are actual explosions.

l. 378: change “these depletions of Hg(0)” to “the depletions of Hg(0) reported here”